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ARTICLE



Expertise, Agreement, and the Nature of Social Scientific Facts or: Against Epistocracy

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ABSTRACT

Taking some controversial claims philosopher Jason Brennan makes in his book *Against Democracy* (Brennan 2016) as a starting point, this paper argues in favour of two theses: (1) There is No Such Thing as Superior Political Judgement; (2) There Is No Such Thing as Uncontroversial Social Scientific Knowledge. I conclude that social science experts need to be kept in check, not given more power.

KEYWORDS

Expertise; epistocracy; social science; economics; epistemic authority

Let me begin with a personal note. This paper was born out of exasperation. My exasperation is caused by a relatively recent tendency among philosophers and other students of science to push to elevate the role of science, in particular of social science, in society. I grew up, philosophically, in a climate that was sceptical of those who aggrandise science, those who maintain that scientific knowledge claims stand apart from all other claims to knowledge, and in particular those who think that scientific experts should be given special powers in society. This kind of scepticism today seems like a thing of the distant past. Instead we are told again to submit to the authority of science (including social science) and to leave technical questions, including technical questions of great social relevance and potential impact, to the scientific experts — because they know what they are talking about.

I think not, and the aim of this paper is to explain why. Though in principle many of arguments apply elsewhere in science as well, I will focus on the social sciences here, of which I am more familiar and which have dramatically increased in social significance in recent times. For specificity I will also focus on an important recent contribution to the debate. The issues discussed therein are entirely general, however. I use it as a peg to hang on my own thoughts about the role of scientific experts in society and that of consensus in science.

I. There is No Such Thing as Superior Political Judgement

My example of an aggrandizement of social science is Jason Brennan's *Against Democracy* (Brennan 2016). Brennan is Robert J. and Elizabeth Flanagan Family Chair and Provost's Distinguished Professor of Strategy, Economics, Ethics, and Public Policy at the McDonough School of Business at Georgetown University. He is also a libertarian (see for instance Brennan 2012; 2014). Now, libertarians have a complicated relationship to democracy. Since the protection of freedoms is their overriding goal, democracy can, at best, be instrumentally valuable. It is valuable (and legitimate) only to the extent that it promotes freedoms and to be avoided (and illegitimate) to the extent that freedoms are curbed. Whether democracy tends to enhance freedoms or curtail them depends on what precisely is meant by the terms, but that democracy cannot be unlimited if freedoms are to

be preserved is fairly obvious and has been discussed in modern political theory at least since Tocqueville's *Democracy in America*.

That democracy might jeopardise freedoms is, however, not Brennan's stated reason to reject democracy. Instead he makes a perfectionist case against democracy. He argues that democracies tend to produce poor outcomes because voters are, on average, incompetent. He also argues in favour of an alternative system of government called 'epistocracy'. While he does not advocate a specific form of epistocracy, all forms have in common that voting power is roughly proportional to the voter's knowledge of social science facts and principles. An epistocracy suppresses the voices of incompetent individuals by banning them from voting altogether, by giving fewer votes to them than to more competent individuals, or by blocking incompetent decisions through a body of experts with veto power. By attenuating the influence of incompetent individuals and strengthening that of competent individuals, epistocracy is argued to lead to better outcomes.

Brennan's arguments against democracy and in favour of epistocracy build on a number of controversial premisses, only some of which are defended by him. He explicitly argues that voters are 'ignorant, irrational, misinformed nationalists', and he provides a fair amount of evidence in favour of this claim (Chapter 2). In fact, the case is much less clear-cut than Brennan makes it appear. The evidence he cites is subject to significant selection bias and many claims leave a number of alternative interpretations open. Take, for instance, the following:

During the 2000 US presidential election, while slightly more than half of all Americans knew Al Gore was more liberal than Bush, they did not seem to understand what the word liberal means. Fifty-seven percent of them knew Gore favored a higher level of spending than Bush did, but significantly less than half knew that Gore was more supportive of abortion rights, more supportive of welfare state programs, favored a higher degree of aid to blacks, or was more supportive of environmental regulation. (Brennan 2016, 62 [page numbers refer to the iBook version]; data from; Somin 2013)

One might call this ignorance. Alternatively, one could postulate that voters are no different from other organisms that live in a complex environment and have to make do with limited cognitive capacities and time to reach a decision. In situations like this, it is not at all rational to collect all potentially relevant information before making a decision. Instead, the organism can use a heuristic that enables it to make a decision that is *good enough* and requires no more information than the organism can handle in the relevant timeframe. In the present case, the organism could use, for instance, the take-the-best heuristic. That heuristic estimates which of two alternatives has a higher value on a criterion by choosing the alternative based on the first cue that discriminates between the alternatives. To decide which of two politicians is preferable a smart voter thus might look at a small number of issues ('cues') that are important to him or her and decide on the basis of the first issue that discriminates between the two. Since these issues may differ from individual to individual, the fact that on average voters don't know where a candidate stands on any given issue is not a sign of individual ignorance. The take-the-best heuristic does pretty well in forecasting election outcomes from information about how voters expect the candidates to deal with the most important issue facing the country (Graefe and Armstrong 2012). Another study found that about 75% of voters in the five U.S. Presidential Elections from 1972 to 1988 'voted correctly' in the sense that they voted for the candidate that they would have chosen anyway if they had possessed full information (Rau and Redlawsk 1997).

Another premiss Brennan argues for explicitly is the existence of objective competence in political matters. He often speaks of 'superior political judgment' (possessed by some individuals but not others) and compares it to the superior judgement of pilots in matters of flying planes, plumbers in matters of plumbing, and surgeons in matters of surgery. This is similar to the argument one often hears in the context of discussing the role of experts in society, one that has been attacked up by Paul Feyerabend a long time ago:

Is it not safer in the case of serious illness to trust the judgement of a physician than the judgement of a witch, and should physicians not therefore receive a special position in our society? (Feyerabend [1976] 1999, 125)

I want to challenge the analogy Brennan makes between political judgement and other types of judgement, which I'll subsume under the term 'technical judgement'. Let us suppose that there is such a thing as superior *technical* judgement. Would that present a good reason to believe that there is also superior political judgement? Political and technical judgement are disanalogous in important ways. In technical judgements, there is a unique or overriding clear-cut goal the expert and his or her client agree on. In Brennan's examples, the respective goals are: arriving at the destination safely and on time, having flowing warm and cold water without leakage, and performing an agreed-upon surgery with the least amount of collateral damage. Even if in some cases trade-offs between subgoals could be construed (say, between 'arriving safely' and 'arriving on time'), one of the subgoals is clearly overriding and stands in a lexical relationship with the other. The opposite is the case in political matters. Here the existence of a multitude of goals is the norm, and there are always conflicts and trade-offs between different goals. It is therefore that the judgement cannot be left to a technical expert.

Moreover, in technical decision problems means-ends relationships are very well understood. This is what makes the problems technical problems. One can learn how to fly a plane safely, how to plumb effectively, how to perform surgeries. Means-ends relationships in political matters are far less well understood. This is in part because the goals themselves are often vague and under-specified. And it is in part because we simply lack the technical knowledge of what measures to implement in order to realise a goal, even in cases this goal is agreed upon and specific. I will come back to this point below.

There are thus three reasons to reject the idea that there is such a thing as 'superior political judgement'. First, an important part of making a political judgment involves trade-offs between different social goals such as freedom vs. equality or wealth vs. security. While an expert might have a better understanding of the nature of these trade-offs, coming down on one side or the other can only be based on values that are in no way more accessible to the expert than the layperson. Second, there is considerable uncertainty in the the application of a value or goal in a concrete context. Is a society in which everyone is poor except for elite party members more equal than one in which the average member is more wealthy but the distribution follows a power law? According to which index? Who determines that that index is the appropriate one to use? Third, there is considerable uncertainty in the means-ends relationships, even when there is agreement on which goals to pursue and what that means in a given context.

A premiss Brennan leaves implicit and undefended is that there is a correlation between 'knowledge of social science' and 'superior political judgement'. (If he didn't presume that there is such a correlation, it would hardly make sense to demand that voters be better educated in social science.) Suppose then, for the sake of the argument, that there is such a thing as superior political judgement. Do we find it among people with high levels of social science knowledge? Brennan surely thinks so:

[...] I justifiably believe that I – a named professor of strategy, economics, ethics, and public policy at an elite research university, with a PhD from the top-ranked political philosophy program in the English-speaking world, and a strong record of peer-reviewed publications in top journals and academic presses – have superior political judgment on a great many political matters compared to many of my fellow citizens, including to many large groups of them. If I *didn't* believe that about myself, I'd feel like a fraud every time I teach a political economy course. (240; emphasis original)

One couldn't make this stuff up if one tried, could one?

Given there is no such thing as superior political judgement, it is hard to show empirically that it does or doesn't correlate with social science knowledge. Now, postulating that there is such a thing for the time being, as I have done, we can also postulate that superior political judgement correlates with success at predicting political events, and we can study whether *that* correlates with social science knowledge in turn.

This is just what Philip Tetlock has done in a 20-year study of expert political judgement (Tetlock 2006). Alas, the results are sobering. The forecasters were 284 experts from a variety of fields,

including government officials, professors, journalists, and others, with many political views, from Marxists to free-marketeers. The tournaments solicited roughly 28,000 predictions about the future and found the forecasters were often only slightly more accurate than chance, and usually worse than basic extrapolation algorithms, especially on longer-range forecasts three to five years out. Forecasters with the biggest news media profiles were especially bad. The research shows that 'The attentive reader of the *New York Times* is likely to be as adept at picking up predictive cues as renowned area study specialists' (87).

Nor is Tetlock's study alone in finding this result. A paper on forecasting the decisions made in conflicts by Keston Green and J. Scott Armstrong concludes:

The people we surveyed expected that forecasting decisions in conflicts would be difficult. Our findings confirmed this. Most respondents nonetheless expected experts to be better forecasters than novices. They were wrong. Expertise did not improve accuracy. Neither experts nor novices did substantially better than guessing. (Green and Armstrong 2007, 293)

This, in turn, is consistent with much older research by one of the two authors that surveyed existing literature on expert forecasting (Armstrong 1980). Many more studies come to similar conclusions.

To be fair to Brennan, he does cite Tetlock (2006), albeit in a different context, and criticises the work on two counts (375–7). He argues that Tetlock never tests experts against laypersons proper because the 'laypersons' in Tetlock's sample were Berkeley undergraduate students. Thus: 'Tetlock was testing the cognitive hyperelites against the cognitive superelites' (376). According to Brennan, Tetlock also biased the study by focusing on questions the experts themselves regard as the 'hard' questions. By contrast, Brennan wants to focus on views experts can all agree on (377).

Other research replicates Tetlock's major results, however, and 'random guessing' is often among the alternatives. The problem is that expert forecasts are hardly more accurate than guesswork. Moreover, Brennan ostensibly argues in favour of epistocracy (government of those with a certain kind of knowledge) rather than aristocracy (government of the best). By comparing experts with Berkeley students, Tetlock controls for factors such as general education and intelligence and is able to address the question whether more subject-specific knowledge improves forecast accuracy. It doesn't. And that is a problem for Brennan's account.

Brennan's second point was that Tetlock's study is biased because it only looks at 'hard questions'. I will argue below that most social science questions are of this kind and that there is very little, if anything, we can all agree on.

II. There Is No Such Thing as Uncontroversial Social Scientific Knowledge

This leads straight to the next premiss Brennan leaves implicit and undefended: that there is such a thing as uncontroversial social scientific knowledge. Let us grant Brennan that 'there is... a wide range of agreed-on views, such as that we should have free trade and avoid price controls' (377). The problem is that such agreement exists, if at all, at best *among mainstream economists*. When we look a little farther afield, for instance to heterodox economists, historians of economics, socio-economists and the like we are very unlikely to encounter agreement. For instance, Ha-Joon Chang writes about the historical track record of free trade policies:

With only a few exceptions, all of today's rich countries, including Britain and the US – the supposed homes of free trade and free market – have become rich through the combinations of protectionism, subsidies and other policies that today they advise the developing countries not to adopt. Free-market policies have made few countries rich so far and will make few rich in the future. (Chang 2010, 101)

According to Chang, a combination of anti-free market policies made rich countries rich. And countries that adopted free-market policies in early stages of development? As Joan Robinson comments on the fate of Portugal vs England:

The imposition of free trade on Portugal killed off a promising textile industry and left her with a slow-growing export market for wine, while for England, exports of cotton cloth led to accumulation, mechanisation and the whole spiralling growth of the industrial revolution. (Robinson 1978, 103)

Neither Chang nor Robinson are mainstream, let alone neoclassical economists. Chang, because he revived the so-called ‘infant industry argument’ is sometimes associated with the German Historical School (one of the members of which, Friedrich List, was the first systematic developer of this argument: List 1841), Robinson with the Post-Keynesian school (though both are a lot more original than these labels suggest). But the point is: whether there is agreement on an economic or social issue or not depends on where we look, and there are no standards according to which we can judge an individual economist’s type and level of expertise that are independent of theoretical and political convictions.

In the social sciences, judgements about legitimate expertise are inextricably bound up with membership in schools of thought, which in turn are often strongly associated with politics. This is certainly true in the field from which Brennan draws most of his examples, economics. It is dubious whether a contemporary Austrian School or Post-Keynesian economist has even interactional expertise in neoclassical economics, let alone contributory expertise (and vice versa). Austrians, Post-Keynesians and neoclassical economists publish in their own respective journals, organise their own conferences and make radically different assumptions about ontology, epistemology, and causality. They will be able to follow the broad strokes of each other’s work but do not normally regard members of other schools of thought as being part of the same discourse and therefore able to contribute expertise.

What makes matters worse is that school membership tends to go along with politics. I have never met a left-leaning Austrian but would be very surprised to meet a Post-Keynesian who is not left leaning. Neoclassical economics, due to its theoretical dearth, includes a broad spectrum of political views, but this does not mean that it is free of politics, or freer than other schools.

More generally, value judgements are never far from the surface of a social scientific investigation (Dupré 2007; Reiss 2008, 2013, 2017). Take Brennan’s free trade example. As we have seen, he argues that there is agreement among (mainstream) economists that free trade is a good thing. Many value judgements need to be made to reach this verdict. Even in a highly simplified two-by-two toy model, within which we can show that free trade leads to an increase in economic product, there are winners and losers. If, to use Ricardo’s original example of the two countries England and Portugal and two goods ‘cloth’ and ‘wine’, the two countries start trading with each other, each will specialise in the production of the good for which it has a comparative advantage (Ricardo 1817, Ch. 7). Thus, England will specialise in the production of cloth and Portugal in the production of wine. This, however, means that there are wine producers in England and sheep farmers in Portugal who are harmed by the arrangement. To claim that free trade is a good thing for England and Portugal means to assume (a) that an increase in economic product is a good thing; (b) that the harms English wine producers and Portuguese sheep farmers experience are morally outweighed by the increase in economic product. Neither assumption can be justified without making value judgements.

Some economists go on to argue that potential losers of free-trade arrangements can be compensated. But the ‘can’ is really important here. It is true, within the model, that the economic product increases, and so money is available for the compensation of losers. However, in practice this rarely happens. Losers are seldom fully (financially) compensated for the loss of their income when new trade arrangements are entered. Moreover, even if they were, financial transfers can hardly compensate for the loss of an occupation in a specific industry. Being a wine maker is something quite different from working in a cloth factory or receiving state benefits.

So a society has to make a choice whether it values the increase in average wealth over the working lives of individuals in industries that are likely to be negatively affected from a trade deal.

No amount of subject expertise will help to address this question. Because interests are involved, we can try to answer it on the basis of how it affects us, or we can try to use Adam Smith's tool of an 'impartial spectator' and answer the question on the basis of moral reflection (Smith 1759/2002). Either way, knowledge of economics won't help, only value judgements will.

The astute reader will long have noticed that Brennan's example is question begging with respect to value judgements because the question whether free trade is *good* for a nation is value laden right from the start. A statement drawn from the heart of positive economics such as 'Free trade raises GDP' won't do any better, however. Values are involved in measuring GDP (e.g. Stiglitz, Sen, and Fitoussi 2010), the theories of rational choice that are often employed in modelling economic relationships (Sen 1993), the testing and acceptance of scientific hypotheses (Rudner 1953), among other things. Though an exhaustive treatment of these arguments concerning fact/value entanglement in positive or predictive social science will have to wait for another day, I do think that they are sound for the most part. 'Free trade raises GDP' is similarly value laden as 'Free trade is good for a nation'.

I should point out that it is in many cases possible to agree on an aspect of a research procedure without *explicit* agreement on a set of value judgements. Economists might, for example, agree that GDP is the correct measure of economic product without explicitly discussing all the evaluative concerns that are involved in constructing and implementing the indicator. Similarly, they could agree that setting the statistical significance level to 5% is the appropriate choice without explicitly trading off the consequences of false positives and false negatives. So it is not the case that every individual scientist faces the problem of making value judgements all the time. However, there is no way to *defend* the appropriateness of this or that measure or of this or that significance level or of this or that conception of rationality without making value judgements. Thus, when challenged, individual scientists or groups of scientists have no choice but to defend their standards by invoking value judgements. And since responding to criticism is essential to science, the making of value judgements is (even if not for each scientist on a daily basis).

Deep fact/value entanglement is but one reason why we should expect economists and other social scientists to disagree about matters of fact. Individuals in any sizeable group will disagree about values, in part for reasons discussed above (see also Haidt 2013). But then if value judgements affect factual beliefs they should disagree about facts as well.

There are further sources of disagreement. Empirical generalisations in the social sciences hold rarely, if ever, universally but are instead highly context-dependent. While it may well be true that free trade does (tend to) spur economic growth for countries at a certain, relatively high level of development, this truth is not incompatible with the existence of another truth, *viz.*, that free trade (tends to) decrease the economic potential for developing countries. More generally, social science generalisations depend on (at least):

- time and place of application (Geoffrey Hodgson calls this the 'problem of historical specificity', see Hodgson 2001);
- whether the focus is on the short-run or on the long-run;
- the choice of contrast;
- the choice of measure or indicator.

Thus, in addition to the issue of applying the generalisation to nations at different levels of development, different historical settings and different cultural contexts, we have to distinguish between short- and long-run effects, might come to different answers depending on whether we contrast free trade with thorough protectionism or with an intelligent mix of free-market and interventionist policies, and depending on what precisely we mean by free trade and whether we examine the effects on GDP or on an indicator that measures a wider concept of social welfare. The problem is that different specifications rarely yield similar results (so what's true of one country or period or cultural context might be false of another, the short-run effect might differ from the long-

run effect etc.), and it's often not clear what the 'right' specification is. Most economists agree that Keynesian fiscal stimuli have a short-run expansionary effect on GDP. But the long-run effects are disputed between Keynesians and conservatives, and Keynes' dictum 'in the long run we are all dead' does not cut much ice for the latter.

I want to emphasise that I do not seek to argue here that there are no facts in the social sciences. Relative to a set of value judgements and a full specification of the question at hand (with respect at least to the four dimensions mentioned above), there may well be a fact of the matter. However, in many contexts social scientists of different convictions will disagree not only about value judgments but also about which of a large number of possible specification is the correct or most relevant specification.

It will be useful at this point to introduce a distinction between two kinds of social science facts. 'Feeble facts' correspond to true answers to fully specified research questions under an agreed-upon set of value judgements. I call them feeble because there is no guarantee that they will continue to hold under a variation of any aspect of the research question or value judgement. 'Robust facts' are those facts that do continue to hold under some variation of aspects of the research question or value judgement. Because it is always possible that a fact is not robust under a variation in value judgements or research question, when a feeble fact has been established, additional evidence needs to be provided to establish the robustness of that fact.

We rarely talk in terms of feeble facts, however. Statements such as 'Free trade is good for a nation' or even 'Free trade boosts GDP' would, if true, correspond to highly robust facts. Two economists can disagree over the existence of a robust fact because one infers its existence from one set of feeble facts, and the other, from another set. Of course, robust facts are more relevant to policy concerns. If a general statement such as 'Free trade boosts GDP' were true, a policy recommendation would follow almost immediately. The relevance of a more specific statement such as 'Free trade policies have helped to boost Victorian England's GDP (as measured by indicator *i*) over the short term, relative to set of alternative policies *p* and assuming the constellation of value judgements *v*' for a policy question is much less clear.

A final source of disagreement is that testing is extraordinarily hard in these fields, in part because there are no agreed-upon evidential standards. Two of the other examples Brennan discusses illustrate this point. Take immigration first. Brennan argues that most Americans are anti-immigration, and that this is due to lack of knowledge of the matter (373; see also Caplan 2007; Somin 2013). If they only knew economics, they'd realise that open borders would be a boon to the American economy:

[T]he consensus among published economic work on immigration seems to be that the restriction introduced by mostly closed borders on labor mobility is the single most inefficient thing governments do. Scholarly articles in economics estimate, on average, that the deadweight loss of immigration restrictions is around 100 percent of world product. (374)

The estimated figure of 'around 100 percent of world product' is indeed supported by evidence, even though the survey Brennan cites actually reports a range of 50–150 percent of world GDP (Clemens 2011, 84). But the estimates are all based on models (and there are only four!) that, much like Ricardo's two-by-two toy model of free trade, make heroic idealising assumptions. Two of the models, for instance, 'assume no differences in inherent productivity of migrants and nonmigrants' (85), which is, well, let us say, optimistic.

Contrast this with left-leaning Oxford economist Paul Collier who presents a different, more nuanced and much broader reading of the evidence. Collier argues that there is an optimum level of immigration in each country beyond which the (economic and social) costs outweigh the benefits (Collier 2013). Since current immigration levels in many Western nations are at or already beyond the optimum, opening borders completely would not only not double their GDP but be detrimental to Western societies, according to Collier.

The point is not to argue that the heroic models are necessarily wrong and that Collier is right or the other way around, but to illustrate that there are different ways to assess the evidence. As Joseph Schumpeter once said, 'Economics is only an observational and interpretative science which implies that in questions like ours the room for difference of opinion can be narrowed but not reduced to zero' (Schumpeter 1942/2003, 107). Brennan's source takes a narrow view of evidence with one target variable and a rigorous model to back up the empirical specification. Collier looks at a variety of economic, social and cultural effects, distinguishes effects on different groups (such as low-skilled vs. high skilled workers) and includes studies by economists, sociologists, historians, psychologists and others. There is no one way of collecting, weighing, interpreting and assessing evidence.

A third example Brennan mentions time and again is price theory and its (alleged) implication that price controls are to be avoided: 'no economically literate person now advocates price controls' (332). Well, minimum wages are of course a form of price controls, and there are armies of 'economically literate persons' who advocate minimum wages. In this case too, it is the evidence that is read differently by economists of different convictions. As Nobel laureate Paul Krugman writes:

Until the Card-Krueger study, most economists, myself included, assumed that raising the minimum wage would have a clear negative effect on employment. But they found, if anything, a positive effect. Their result has since been confirmed using data from many episodes. There's just no evidence that raising the minimum wage costs jobs, at least when the starting point is as low as it is in modern America. (Krugman 2015)

The 'Card-Krueger study' was a natural experiment in which one U.S. state (New Jersey) increased the minimum wage while a neighbouring region with which it is highly economically integrated (Western Pennsylvania) stayed put. David Neumark and William Wascher survey the literature using a broader view of evidence and sum up their findings as follows:

Three conclusions, in particular, stand out. First, as indicated in chapter 3, the literature that has emerged since the early 1990s on the employment effects of minimum wages points quite clearly – despite a few prominent outliers – to a reduction in employment opportunities for low-skilled and directly affected workers. (Neumark and Wascher 2008, 286)

Thus, even if there was agreement on values (a big if!) as well as on the specification of the research question (another big if), and there is a feeble fact corresponding to the true answer to our completely specified research question, there is no guarantee that social scientists would come to an agreement about the answer because they lack the means to find the true answer in an uncontroversial fashion. And even if they had found it, disagreement will remain about policy issues. Suppose, for instance, that Chang and Robinson are right in their assessment that relatively large nations at a relatively low stage of development are ill-advised to enter into free-trade agreements with more developed countries. Suppose also that some nations have benefitted from entering trade agreements. These facts hardly determine what the answer is to the question whether contemporary Turkey should enter such an agreement. Is contemporary Turkey more like 19th century England or more like 19th century Portugal?

If all of this is true, how can Brennan defend his view that 'there is also a wide range of agreed-upon views'? Citing libertarian economist Bryan Caplan, he even argues that such agreements are independent of political views:

Caplan is careful to note when economists agree about matters that are not explained by their background ideologies. So, for instance, left-wing, right-wing, moderate, and libertarian economists all support free trade. It's not their overall ideologies that are at work here but rather the fact that they understand and accept mainstream economics. (Brennan 2016: 372)

The reason why it appears to Brennan and Caplan that there is consensus on a controversial issue such as free trade is that they don't look far enough. Perhaps justifiably. Joan Robinson is long dead, and Ha-Joon Chang is a highly heterodox (i.e. 'non-mainstream') economist. The German Historical School whose 'infant-industry argument' against free trade for developing nations Chang has revived, is long forgotten. And yet, what I have said above about the nature of social science

knowledge indicates that the existing consensus in mainstream economics (if Brennan and Caplan are right) exists due to conformism.

Conformism can have a variety of sources. Here are some:

- *Common acceptance of a bad theory.* This might explain the high degree of subscription to Ricardo's free trade theory by contemporary mainstream economists. Almost every economist teaching at a highly ranked university in the U.S. and elsewhere has been brought up on the same set of neoclassical principles that is but one school of economics and in no way exhausts economic ideas (Chang 2014). That neoclassical economics is dominant in the profession today is largely an accident of history (Yonay 1998). At any rate, what should be obvious is that neoclassical economics is not without alternatives.
- *Common acceptance of a bad methodology.* 'Evidence-based' paradigms are dominant in contemporary medicine and social policy. The 'evidence-based' label is used to describe a methodology that regards randomised trials as the gold standard for causal inference and downplays evidence generated by any other method. Most philosophers of science who write on the topic are highly critical of the evidence-based movements. And yet, because they dominate large fields within medicine and social policy, it is certainly conceivable that a knowledge claim becomes generally accepted because it seems to be supported by randomised studies, but it would not be accepted if a broader view of evidence had been taken.
- *Selection bias.* It doesn't take much imagination to suppose that the members of some scientific fields self-selected into these fields on the basis of certain beliefs about the world or certain moral values. Let us just say that there aren't many conservatives in gender studies, or subscribers to certain claims (such as 'there are two biological sexes' or 'biology has an influence on behaviour') which, among biologists, are uncontroversial.
- *Bad social values.* Up until the 1960s the Diagnostic and Statistical Manual of Mental Disorders (DSM) classified homosexuality as a mental disorder, transsexuality still is so classified (as gender dysphoria). There is no doubt that social values have affected these classifications, and when the values held by scientists are not or no longer held in society, a scientific consensus on a matter will be of little relevance to society at large.

For these reasons I reject any attempt, on the part of scientists themselves or of philosophers or any other students of science, to strengthen the role of experts in society. Experts need to be kept in check, not given more power. Scientific conclusions – theories, concepts, facts – are enormously useful for individual and political decision making, but only if they are regarded as that: tools for thinking (and not as commands for action).

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